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UNITED STATES ATOMIC ENERGY COMMISSION

VOLUME XVIII

In the Matter Of:

J. ROBERT OPPENHEIMER

HISTORIAN'S OFFICE  
INVENTORY CONTROL

Job 1193

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UNITED STATES ATOMIC ENERGY COMMISSION  
PERSONNEL SECURITY BOARD

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In the Matter of :  
J. ROBERT OPPENHEIMER :  
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Room 2022,  
Atomic Energy Commission,  
Building T-3,  
Washington, D. C.  
Wednesday, May 5, 1954.

The above entitled matter came on for hearing,  
pursuant to recess, before the Board, at 9:30 a.m.

## PERSONNEL SECURITY BOARD:

MR. GORDON GRAY, Chairman.  
DR. WARD T. EVANS, Member.  
MR. THOMAS A. MORGAN, Member.

## PRESENT:

ROGER ROBB, and  
C. A. ROLANDER, JR.

J. ROBERT OPPENHEIMER.  
LLOYD K. GARRISON,  
SAMUEL J. SILVERMAN, and  
ALLAN B. ECKER, Counsel for J. Robert Oppenheimer.  
HERBERT S. MARKS, Co-counsel for J. Robert Oppenheimer.

BY MR. SILVERMAN:

Q Dr. Teller testified about a board of four people at the end of the war, or near the end of the war, who he understood decided that the thermonuclear program should not be pushed. Can you cast some light on that?

A I think I can. I think I know what Dr. Teller was talking about.

There was a panel of four people. Their names were Arthur Compton, Ernest Lawrence, Enrico Fermi and me, Robert Oppenheimer. We had been asked to advise on the use of the bombs, on the general nature of the future atomic energy program, but we were asked specifically through Mr. Harrison, on behalf of the Secretary of War,, to prepare as detailed an account as we could of everything we knew that could be done or needed doing in the field of atomic energy.

This was not just military things. It involved the use of isotopes and the power problem and the military problems. As a part of this report, we discussed improvements in atomic weapons and in the carrier problem. As a part of this report, we discussed the thermonuclearbomb, the Super, as it was called. That was all we had in mind then. I believe that section was written by Fermi. I believe that Dr. Teller correctly testified that his own view on what the problem was, was attached as a slightly dissenting or even strongly dissenting view to our account.

We wrote an account which was not a recommendation of policy at all, as I remember, but was an analysis of where we thought the matter stood. I think General Nichols' letter to me quotes from it, and says this program did not appear on theoretical grounds as certain then as the fission weapon program had at some earlier stage. This was a rather long and circumstantial account of what we knew about it. It was not intended and was not a statement of what should be done. It was an assessment of the technical state of the problem.

This board had no authority to decide, it was not called on to recommend a decision, it did not decide nor recommend a decision. It described. I think Dr. Teller was a little mistaken about what our function was.

Q Dr. Alvarez mentioned the expression "a gram of neutrons per day" --

MR. ROBB: How is that?

MR. SILVERMAN: "A gram of neutrons per day". I think you will find that in his diary.

MR. ROBB: Do you have the transcript on that?

MR. SILVERMAN: I have it.

MR. ROBB: I recall the expression of a gram of neutrons, but the "per day" I don't recall.

MR. GRAY: I think it would be well to indicate more precisely --

DR. EVANS: I think he said "a gram of neutrons per



day."

MR. ROBB: Maybe he did. I don't know. It just struck me. He may well have said it. I would just like to be able to look at it.

MR. GRAY: Let us hold a minute.

MR. SILVERMAN: I think I can find it, sir.

On October 9th in the diary. I will say that there the expression in the diary is just "gram of neutrons recommended".

MR. ROBB: That is right.

MR. SILVERMAN: Let us see what the testimony is.

MR. ROBB: It may well be, Mr. Silverman. I don't know. What was that date in the diary?

MR. SILVERMAN: October 9. Page 2661 of the transcript. It is on page 2662 Dr. Alvarez said, "People agree that the idea of radiological warfare was attractive in many ways, but again the country had no supply of free neutrons and in order to make these radioactive agents, at least a gram of free neutrons per day would be needed. The military could hardly become enthusiastic about a program that could not be initiated unless pile s of this type could be built. We felt that they appreciated the usefulness of this method of warfare, but thought it was so far in the future as not to cause them any immediate concern."

MR. ROBB: Then I asked the question, "Gram of

neutrons recommended, is there any comment to make about that."

Go ahead. I am sorry. I just wanted to get it precisely.

MR. SILVERMAN: I don't see that it adds any more to the gram per day.

MR. ROBB: It is all right. It is on the record, anyway.

MR. SILVERMAN: That refers to the entry in Dr. Alvarez's diary of October 9, 1949.

BY MR. SILVERMAN:

Q Now, as I say, Dr. Alvarez in his testimony used the expression, "a gram of free neutrons per day", and in his diary he talked something about a gram of neutrons.

Do you know where that phrase first appeared in the official literature?

A I have not read all the official literature. In the summer of 1948 I was Chairman of a panel of the Committee on Atomic Energy of the Research and Development Board, which met in Berkeley. I think the members of that Board were all military, but we consulted with civil experts. In that report, I for the first time wrote that we needed at least a gram of free neutrons a day, and the facilities for that were required for military purposes. That was the summer of 1948.

I will not say that no one had written it before.

I never saw it before, or heard it before.

Q I would like to ask one question off the record because I hate to dispose my ignorance. Is that a lot of neutrons?

A By then existing standards it seemed an awful lot of neutrons.

MR. GRAY: I think it is useful to have it in the record if this point is meaningful.

MR. SILVERMAN: All right.

BY MR. SILVERMAN:

Q Between January 1947 and January 1950, which is the first three years of your chairmanship of the GAC, how many new reactors were started by the Atomic Energy Commission?

A This would be better found by reading the Commission's reports, and I have not done so. This work was very slow to get started, but if you include all kinds of reactors, for development, for research, and for production, perhaps around eight.

Q And did the GAC express its views to the Commission about the slowness of getting started?

A The GAC wrote reams on the subject of getting the reactor program off the dime. The reams may not have been very sensible, but they were clearly addressed to this problem.

Q Dr. Libby and Dr. von Neumann are now members of the GAC, are they not?

A Yes, they are.

Q And they are both enthusiastic proponents of the hydrogen bomb?

A Yes, they are. I believe today everybody is an enthusiastic proponent.

Q But were they when they were appointed?

A Yes, they were.

Q Did you have anything to do with their appointment?

A I don't know. The appointments were Presidential. I did, however, include the names of von Neumann and Libby on the list, I believe, of five names that I submitted to Mr. Dean in the summer of 1950.

I should for completeness say that the other people on that list, as I recollect, though very competent, were not identified with enthusiasm for the hydrogen bomb. Bacher, Fermi, and Bethe were also on the list. Libby was appointed in the summer of 1950. von Neumann was not, but he was appointed as soon as a vacancy appeared through the resignation of Dr. Cyril Smith. Both men served on the GAC for a while while I was Chairman.

Q General Wilson testified, I believe, that at some stage you did not support the installation of two of the three methods of long range detection. Did you ultimately support those two methods?

A Yes.

Q And was your decision about supporting the

installation of those two or three methods made on the basis -- on what basis was it made?

A This is not recollection.

MR. GRAY: This is not what?

THE WITNESS: This is not a recollection. The only ground for holding up the installation of something is doubt as to whether its development had reached the right stage for it to be effective. That is the best answer I can give to you.

BY MR. SILVERMAN:

Q As to the third method, the one you did support, do you recall the circumstances of the initiation of that method?

A Yes, I do. This was just after Hiroshima, and we developed at Los Alamos -- I believe that the man directly in charge was Kenneth Bainbridge -- what we hoped might be an effective long range detection device. I directed that we try this out with the cooperation of the Air Force, and we did succeed in identifying and describing the Hiroshima explosion by flights over the continental United States.

Later, when I was on the General Advisory Committee, I believe the committee wrote something to this effect, that the problem of detection of foreign explosions was of unparalleled importance. That since this was not clearly a Commission problem, we did not insist on being informed of the progress of the work, but we wished to record our view

that progress was urgent and important. It was in the Defense Department that I had a more direct connection with the development of this method. It was completely successful in detecting and describing the first Soviet explosion, at least the first one we know about.

DR. EVANS: That was radiation detector, was it not?

MR. ROLANDER: I don't think we should discuss that.

THE WITNESS: I am sorry.

DR. EVANS: Excuse me.

BY MR. SILVERMAN:

Q Of the three methods, was that first method the one that has furnished the most significant and important information, as far as you know, or is that classified?

A Let me say simply that it has furnished an enormous amount of information which is technically very valuable. For some purposes the other methods are quite useful in giving supplementary data. I think I can't go further.

Q Dr. Alvarez testified that at a meeting of the Military Objectives Panel in about December 1950, you said something to the effect that "We all agree that the hydrogen bomb program should be stopped, but to do so will disrupt the people at Los Alamos and other laboratories, so let us wait for the Greenhouse tests, and when those fail that will be the time to stop the program. Can you cast any light on that?

A I am clear as to what my views were, and therefore

fairly clear as to what I would have said, which resembles to some extent what Dr. Alvarez recounted. I did not think the Greenhouse test would fail. It was well conceived technically, and there was no ground for such an opinion. With one possible exception, and barring tests conducted recently of which I don't know, no United States test of an atomic device has ever failed in the sense that what went on deviated markedly and negatively from theoretical prediction. I could not have said that I expected it to fail, because I didn't think it would, and I could not have said that I expected it to fail, because this sort of statement about a test is something none of us ever made. The reason for making the test was that we wanted to find out.

What I did believe, and for the wisdom of this view I am not making an argument, was that the real difficulties with the Super program, as it then appeared, were not going to be tested by this Greenhouse test; that the test was not relevant to the principal question of feasibility. I am fairly sure that in the course of discussions at the panel, we would have commented on this.

On the question of where the Super program stood, on the relevance of that to the Greenhouse test, of the doubts that I felt as to whether this part of the Greenhouse test was a sensible thing technically to do, I would have said that to stop this part of the Greenhouse test, even though it

made no technical sense, would be disruptive and destructive of all parts of the Los Alamos program.

I think that is the true story of what I would have said at this panel meeting, and Dr. Alvarez's recollection is in some respects mistaken.

Q What were your views as to the feasibility of the Super at that time?

MR. ROBB: What time are we talking about?

MR. SILVERMAN: This is December 1950, at the time of the Military Objectives Panel.

THE WITNESS: On the basis of then existing ideas it was highly improbable that this could be made; that we needed new ideas if there was to be real hope of success.

May I add one comment? In actual fact this component of the Greenhouse test had a beneficial effect on the program. This was in part because the confirmation of rather elaborate theoretical prediction encouraged everybody to feel that they understood and when they then made very ambitious inventions, the fact that they had been right in the past gave confidence to their being right in the future.

It may also to a smaller extent have provided technical information that was useful. Certainly its psychological effect was all positive. It would have been a great mistake to stop that test.

BY MR. SILVERMAN:



Q And you thought so at the time and said so?

A But not for the right reasons.

Q There have been discussions on your views on continental defense and tactical and strategic use of weapons and so on. Perhaps if we could do this very briefly, could you give very briefly your views on continental defense?

A As of when?

Q As of now, if you like. As of the last year or two.

A If the Board is not saturated with this, I will say a couple of sentences.

Q As of the time of the Lincoln study.

A The immediate view after the war was that defense against atomic weapons was going to be a very tough thing. The attrition rates of the Second World War, though high, were wholly inadequate to this new offensive power.

By the attrition rates, you mean the number of attacking airplanes you could shoot down and kill?

A Precisely. In the spring of 1952, the official views of what we could do were extremely depressing, 10-20 per cent, and there were methods of attack which appeared to be quite open to the enemy where it was doubtful that we would either detect or intercept any substantial fraction of the aircraft at all.

I knew that on some aspects of the defense problem, valuable work was in progress at Lincoln and elsewhere. I

knew something of the Charles study. I thought that over the next, say, few years, four years, our defensive capability could and would rise perhaps to the point where 30 or 40 per cent of incoming aircraft would actually be shot down. I thought as enemy capabilities developed, and above all if really long range ballistic rockets, ballistic missiles came into the picture, the picture would get bleak again, and therefore I tended to draw on the blackboard a curve which was very low at that time, which rose to a maximum of maybe five or eight years later, and which then fell down again as offensive capabilities increased.

I had heard enthusiastic tales about early warning and about remote intercept largely from Berkner and through Rabi from Berkner. I thought the views presented were exaggeratedly optimistic with regard to remote interceptor. I had become aware of the importance for air defense, for civil defense, and for SAC for strategic air, of the best early warning we could get.

I think today I am along with everyone else clear that the early warning problem can be solved in a satisfactory way. I am not clear that remote intercept is a sensible or possible thing. My view is that this is by no means a happy situation, and I know of no reason to think that it ever will be a happy situation, but that the steps that are now being taken and others that will come along as technology develops

are immensely worth taking if they only save some American lives, if they only preserve some American cities, and if they only create in the planning of the enemy some doubt as to the effectiveness of their strikes. I don't know whether this answers the question.

Q I think that answers the question.

A I have never gone along with the 90-95 per cent school. I hope they are right, but I have never believed them.

Q The 90-95 per cent school is the school - -

A That thinks you can eliminate practically all of the enemy attack.

Q What did you conceive to be the relation between continental defense and strategic air power?

A First, strategic air power is one of the most important ingredients of continental defense. Both with the battle of Europe and with the intercontinental battle, clearly the best place to destroy aircraft is on the ground on enemy fields, and that is a job for strategic air power.

Second, at least the warning elements and many of the defensive elements of continental defense are obviously needed to protect the bases, the aircraft, which take part in the strategic air campaign. This is the two-way relation which I think has been testified to by others. This has always been my understanding.

Q It has been suggested that perhaps you had more interest in the tactical than the strategic use of atomic weapons. Could you comment on that?

A It has been talked about a great deal. When the war ended, the United States had a weapon which revolutionized strategic air warfare. It got improved a little. The Air Force went hard to work to make best possible use of it.

For eight or nine years the atomic bomb and the planes of the Strategic Air Command, and its base system have been the greatest single component of our military effort, and I think the only offensive component that amounts to anything. Even during the Second World War we had a request through General Groves from the Army as to whether we could develop something that would be useful in the event of an invasion of Japan to help the troops that would be faced with an entrenched and determined enemy. The bomb that was developed and embellished in the years 1945 to 1948, and the aircraft that go with it, the whole weapons system, can of course be used on any target, but it is a very inappropriate one for a combat theater. Therefore, there was a problem of developing the weapon, the weapon system, the tactics to give a new capability which would be as appropriate as possible under fire, and in the combat theater. This is not because it is more important. Nothing could be more important than the armament that we had, and which is now to be extended, perhaps to some extent

superseded, by thermonuclear weapons. It was simply another job which needed doing, and which is not competitive, ought not to be competitive any more than continental defense is, which is another part of the defense of the country and of the free world. That job was slow in accomplishment. It is accomplished now, or largely accomplished now.

MR. SILVERMAN: I have no further questions of Dr. Oppenheimer.

MR. GRAY: I wonder if you have any, Mr. Robb?

MR. ROBB: I have a few, yes, sir.

#### CROSS EXAMINATION

BY MR. ROBB:

Q Doctor, I want to show you a carbon copy of a letter dated September 20, 1944,, addressed to Dr. R. C. Tolman, 2101 Constitution Avenue, Washington, D. C., bearing the typewritten signature, "J. R. Oppenheimer", and ask you if you wrote that.

MR. SILVERMAN: May I look at it?

MR. ROBB: I am sorry, it is declassified with certain deletions which have just been circled here.

THE WITNESS: I am sure I wrote it. Would you give me the courtesy of letting me read it?

BY MR. ROBB:

Q You mean read it aloud?

A No.

Q Sure, that is why I showed it to you.

A I remember the circumstances.

Q Have you read it now?

A Yes.

Q Including the portions that were circled?

A Right, which I think are relevant to the sense of the whole letter.

Q Doctor, do you think if we read this into the record that you can paraphrase those portions in some innocuous way?

A Let us see how it goes.

Q It doesn't seem to be very much, and we did that once before.

MR. ROBB: Mr. Chairman, might I ask to have this read by Mr. Rolander? When you get to the portions that are deleted --

MR. SILVERMAN: I really find this a very disturbing procedure.

MR. GRAY: All right, you can state your concern.

MR. SILVERMAN: My concern is that here on what I hope is the last day of the hearing we are suddenly faced with a letter which I have not seen, which I know nothing about, and which is going to be read into the record, and I haven't the vaguest idea of what it is about.

THE WITNESS: It is from my file.

MR. SILVERMAN: There are lots of things in the file.

MR. ROBB: Mr. Chairman, Dr. Oppenheimer testified, as I understood his testimony, to certain opinions which were expressed to him, and I think by him in the period 1944-45, about the thermonuclear.

THE WITNESS: No.

MR. ROBB: I think there were certain discussions he had with Groves and others.

THE WITNESS: In 1945?

MR. ROBB: In 1945, yes..

I think the letter pertains to that general subject. I think the Board ought to have the letters before the Board.

MR. GRAY: There seems to be no question about this is a letter written by Dr. Oppenheimer. I believe he has identified it.

I repeat, Mr. Silverman, what I have said many times, and what I hope has been demonstrated by the conduct of this proceeding, that if you are taken by surprise by anything that happens in this procedure, we will give you an opportunity to meet a difficulty arising.

MR. SILVERMAN: At this moment I haven't any idea whether I am going to be taken by surprise. I do think it would have been a very easy matter to give us a paraphrased copy of this letter in advance.

MR. ROBB: Mr. Chairman, until Dr. Oppenheimer testified about this this morning, we had no idea that this

letter would become relevant at this particular time. If Mr. Silverman does not want Dr. Oppenheimer to have a chance to comment on the letter, that is all right with me.

MR. SILVERMAN: I really think that is not the question at all. The real question that I suggest is that it would have been a very easy thing to let us have some intimation of what this is about, instead of having it just flounder here -- I don't know whether we are caught by surprise or not. I don't know what we are talking about.

MR. ROBB: You know, Mr. Chairman, it seems to me that Mr. Silverman is most anxious to be outraged. I don't know why.

MR. SILVERMAN: Mr. Chairman, is that remark to remain on the record?

MR. GRAY: I know we have had frequent exchanges between counsel which are on the record.

MR. SILVERMAN: The suggestion that I am anxious to be outraged suggests that I am putting on some kind of an act--

MR. ROBB: Mr. Chairman, there is some suggestion that I have done something improper in anticipating what Dr. Oppenheimer is going to testify.

MR. SILVERMAN: I frankly am about documents being produced that we have not seen and being produced at the last minute. This is an inquiry and not a trial, and it would not happen at a trial. I still don't know what is in this



document. For all I know it is a very helpful document.

MR. GRAY: It may well be. The Chairman of the Board makes this statement, that while this is an inquiry and not a trial, there are involved in this proceeding counsel who have not always agreed. I think I can speak for my colleagues on the Board when I say that this Board takes cognizance of this fact, and the fact that observations of counsel appear on the record do not in any way indicate agreement or disagreement on the part of this Board with observations by counsel. As far as producing the testimony here has been concerned, there has been the greatest amount of latitude afforded both to Dr. Oppenheimer and his counsel and to Mr. Robb throughout. I must say that I don't think frankly that the observations of counsel on either side are matters which will be of too much interest and concern to this Board. I suggest that you proceed, Mr. Robb.

MR. ROBB: Would you go ahead and read it?

MR. ROLANDER: I will hand Dr. Oppenheimer a copy of this letter.

THE WITNESS: Is this an unexpurgated copy?

MR. ROLANDER: It has the portions that are classified circled. The letter is dated September 20, 1944, addressed to Dr. R. C. Tolman, 2101 Constitution Avenue, Washington, D. C.

"Dear Richard:

"The accompanying letter makes some suggestions about procedure in the matter of Site Y recommendations for postwar work. As you will recognize, the problem of making sensible recommendations is complicated by the fact that we do not know how far this project will get during its present life. It seems a reasonable assumption that we will succeed in making some rather crude forms of the gadget per se, but that the whole complex of problems associated with the super will probably not be pushed by us beyond rather elementary scientific considerations.

"I should like, therefore, to put in writing at an early date the recommendation that the subject of initiating violent thermo-nuclear reactions be pursued with vigor and diligence, and promptly. In this connection I should like to point out that gadgets of reasonable efficiency and suitable design can almost certainly induce significant thermo-nuclear reactions in deuterium even under conditions where these reactions are not self-sustaining" --

Then there is a portion that has been deleted.

BY MR. ROBB:

Q Can you paraphrase that for us, Doctor?

A Yes. It is a part of the program of Site Y to explore this possibility.

MR. ROLANDER: Continuing, "It is not at all clear whether we shall actually make this development during the

present project, but it is of great importance that such" -- and then there is a blank.

THE WITNESS: I think that can just be left out.

MR. ROLANDER: "such blank gadgets form an experimentally possible transition from a simple gadget to the super and thus open the possibility of a not purely theoretical approach to the latter.

"In this connection also I should like to remind you of Rabi's proposal for initiating thermo-nuclear reactions"-- and then blanks.

BY MR. ROBB:

Q Do you want to paraphrase that, Doctor?

A "Without the use of a fission trigger."

MR. ROLANDER: "At the present time Site Y does not contemplate undertaking this, but I believe that with a somewhat longer time scale than our present one, this line of investigation might prove profitable.

"In general, not only for the scientific but for the political evaluation of the possibilities of our project, the critical, prompt and effective exploration of the extent to which energy can be released by thermo-nuclear reactions is clearly of profound importance. Several members of this laboratory, notably Teller, Bethe, von Neumann, Rabi and Fermi have expressed great interest in the problems outlined above and I believe that it would be profitable to have a rather

detailed discussion of the present technical status (which I know to be confused) which should be made available to the committee before it draws up its final recommendations.

"Sincerely yours, J. R. Oppenheimer."

BY MR. ROBB:

Q Doctor, before we go into any discussion, I will show you a carbon copy of another letter dated October 4, 1944, addressed to Dr. R. C. Tolman, 2101 Constitution Avenue, Washington, D. C., bearing the typewritten signature, J. R. Oppenheimer", and ask you if you will read that and tell us if you wrote it.

MR. SILVERMAN: Is this a continuation of the same correspondence, Mr. Robb?

MR. ROBB: Yes, I think so. I am trying to get this unclassified so I can hand you a copy of it, Mr. Silverman.

MR. MARKS: When was this document unclassified that you are about to hand to us?

MR. SILVERMAN: It is being declassified now.

MR. MARKS: I think we are entitled to an answer to that question.

MR. ROBB: How is that again?

MR. MARKS: The question is when was this document unclassified?

MR. ROBB: I haven't any idea. Do you know, Mr.

Rolander?

MR. ROLANDER: It may appear on the face of the document.

MR. ROBB: There is a note on there. I don't know when it says.

MR. ROLANDER: Just a minute.

THE WITNESS: I have read the letter.

MR. ROBB: Does it say on there when it was unclassified?

THE WITNESS: 4-13/54.

MR. ROBB: Have you a copy of that for Mr. Silverman?

THE WITNESS: I will recognize the letter as one that I wrote.

MR. ROBB: We are handing you a copy of that last letter, Mr. Silverman.

BY MR. ROBB:

Q You testified that is a letter you wrote, Doctor, or rather a copy of a letter you wrote.

A I have no reason to doubt it whatever.

MR. ROBB: Did the Chairman wish me to wait until counsel have had a chance to look at this before it is read or could they follow it as it is read?

MR. SILVERMAN: I will request that.

MR. GRAY: All right. We will wait until they get a chance to look at it.

MR. SILVERMAN: We are ready.

MR. ROBB: Would you read it, Mr. Rolander?

MR. ROLANDER: The letter is dated October 4, 1944, addressed to Dr. R. C. Tolman, 2101 Constitution Avenue, Washington, D. C.

"Dear Dr. Tolman:

"In transmitting to you the recommendations of workers at Project Y on the technical and scientific developments which should be supported in the postwar period, it would seem unnecessary, in view of the essential unanimity in detail and in emphasis, to provide a summary of our opinions. I should like, however, to emphasize a general point of view which I believe is shared by most of the responsible members of the project" --

DR. EVANS: Of this project.

MR. ROLANDER: "-- of this project, but which deserves repeated and clear statement.

"It may be difficult for those not directly associated with the efforts of Project Y to appreciate how provisional, rudimentary and crude they have been. I regard this not primarily as criticism of the project, but as an inevitable consequence of our attempt to meet a directive with the greatest possible speed. This has for instance made it impossible for us to embark on methods of assembly and use which require long experience with the active materials

It has furthermore discouraged us from entering into a program of more than the minimum complexity. I believe that these limitations have all been appropriate for this war-time project. What is essential is that they should not be forgotten in evaluating future prospects.

"To make these points somewhat more concrete, it is extremely unlikely that Project Y, even if completely successful in its present program, will produce weapons whose explosive effect is equivalent to more than about 10,000 tons of high explosive. It would seem unlikely that we will manage to design weapons in which the efficiency of the reaction is as much as ten per cent. It is almost certain that we shall not in a practical way explore the possibilities of releasing the vastly greater energies available in self-sustaining thermo-nuclear reactions which should afford energy release some ten thousand times greater than those from presently contemplated designs. Finally, the methods of assembly actually being pursued by this laboratory are complicated, crude and bulky, and we shall probably not develop methods which by incorporating autocatalytic features in assembly may completely alter the nature and difficulty of the problems of delivery.

"The above are specific indications of directions which we now know to be worthy of further research. No one can have witnessed the rapid development of ideas in this project, and

the extreme lability of fundamental design, without appreciating that the work of this project constitutes a beginning in a field of great complexity and great novelty. Only when investigations can be pursued in a more leisurely and scientifically sound manner than is possible in war, and only when actual experience with the active materials can be used to supplement theoretical ideas of their behavior, will it be possible to foresee the boundaries of this new field.

"The above considerations are all intended to focus attention at one point. Such technical hegemony as this country may now possess in the scientific and technical aspects of the problem of using nuclear reactors for explosive weapons is the result of a few years of intensive but inevitably poorly planned work. This hegemony can presumably be maintained only by continued development both on the technical and on the fundamental scientific aspects of the problem, for which the availability of the active materials and the participation of qualified scientists and engineers are equally indispensable. No Government can adequately fulfill its responsibilities as custodian if it rests upon the war-time achievements of this project, however great they may temporarily seem, to insure future mastery in this field. I believe that this point is one which will readily be appreciated by the members of your Committee, but that it is my duty as the Director of the project directly concerned with these



developments, to insist on it in the clearest possible terms.

"Sincerely yours, J. R. Oppenheimer."

MR. SILVERMAN: Just one second. Do we now have the complete correspondence between Dr. Oppenheimer or Dr. Tolman on this matter, or are there more letters?

MR. ROBB: I haven't the slightest idea whether there were more letters written or not. These are the ones that are available to me now. I may say I never read these letters until this morning myself.

MR. SILVERMAN: Thank you.

BY MR. ROBB:

Q Doctor, who was Dr. Tolman?

A He was a very close and dear friend of mine. He had been Vice Chairman of the National Defense Research Committee. When I assumed the responsibility for Los Alamos I introduced him or saw that he was introduced to General Groves. General Groves asked him to be one of his two scientific consultants. He was a member, possibly secretary of the Committee of Review, which visited Los Alamos in the spring of 1943, and pointed out some things that we needed to do if we were to be a successful laboratory. He was a frequent and helpful visitor to Los Alamos throughout the war. He was at one time, and I would assume at the time these letters were addressed to him, a member of a committee, possibly chairman of a committee appointed by General Groves.

which was a precursor to the scientific panel to the Interim Committee in trying to sketch out for the benefit of the government what the postwar problems in atomic energy might be. These included military and non-military problems.

I think that these letters were addressed to him in that capacity.

Q And Site Y was what?

A Los Alamos.

Q Doctor, have you any comment you wish to make on these letters, and if so, will you please do it?

A I have a couple of comments. Let us take the first letter, the one of September 20. In the second paragraph, the second sentence -- do you have a copy of this?

MR. SILVERMAN: No.

THE WITNESS: I will read it: "In this connection I should like to point out that gadgets of reasonable efficiency and suitable design can almost certainly induce significant deuterium reactions even under conditions where these reactions are not self-sustaining."

That turned out not to be true, and I think it was known by the end of the war.

In the third paragraph it says, "In this connection also I should like to remind you of Rabi's proposal for initiating thermonuclear reactions" and I paraphrased "without the use of a fission bomb trigger."

"At the present time Site Y does not contemplate undertaking this, but I believe that with a somewhat longer time scale than our present one this line of investigation might prove profitable."

This has been under investigation at Los Alamos both immediately after the war and very recently.

On the general character of the recommendations or views, especially on the second letter, this is the point I made in the testimony before the Stimson Committee, that we were at the very beginning. The comments on how successful a wartime effort would be were too conservative. We did substantially better than was here indicated, but the warning that however it looked, it was not right to rest on it was one that I repeated then. I think that we went over all the points that are mentioned in these letters in the report of the Scientific Panel to the Secretary of War's Interim Committee. I would think that we went over them in the most careful and complete way that we could. These were some comments.

MR. SILVERMAN: What was the date of Secretary Stimson's Interim Committee, approximately?

THE WITNESS: Which dates do you want?

MR. SILVERMAN: The date they started.

THE WITNESS: I don't know when they started, but the date that the panel appeared with them was the first of June,

1945; the date of filing of this long report to which reference has already been made was perhaps October 1945.

BY MR. ROBB:

Q Had you completed your comment on these letters?

A I may need to come back to them, but that is what comes to mind at the moment.

Q At the time you wrote these letters, you were in favor of going ahead with a program for the development of a thermonuclear weapon, weren't you?

A The letters speak for themselves. I believe they speak exactly what I meant.

Q Did you mean that?

A I meant these letters.

Q Did you mean that you were in favor of going ahead with the thermonuclear?

A I would like to read the phrases.

Q What I am getting at, Doctor, laying aside the technical language, wasn't that the ordinary meaning of that you said, that you thought you ought to get busy on the thermonuclear?

A Among other things.

Q Yes.

A With the exploration of the thermonuclear.

Q Did there come a time when you changed that view in subsequent years?

A Manifestly by October 29, 1949, I was saying very different things.

Q Yes. Doctor, something was said about the liquid hydrogen plant at Los Alamos. That was constructed for the purpose of working on a fusion weapon, wasn't it, or hydrogen weapon?

A For preliminary research on ingredients that we thought would be essential in a hydrogen weapon.

Q Yes. In the matter of reactors, there are various kinds of reactors, aren't there?

A Indeed there are.

Q Those built for commercial purposes, those built for research purposes, and those built for production of weapons purposes, isn't that right?

A I have yet to see one built for commercial purposes but I hope I some day will.

Q I am asking for information.

A There are, as I testified, reactors for the development of reactors, reactors for production, reactors for research, and reactors that serve more than one purpose.

Q You were asked about how many reactors were built during your tenure as Chairman of the GAC and I think you said nine, was it?

A No. I think you asked me during the entire period how many were started, and I think I said about a dozen and a

half. Mr. Silverman asked me up to the first of 1950 how many were started, and I said perhaps eight.

Q Were those eight built for research or production?

A This is better found in the reports of the Commission. I believe that three or four were reactor development reactors, namely, to improve the art of reactor development. A couple, two or three were for supplementary production, and two or three were for research.

Q Was any of them a so-called heavy water reactor?

A No. I am not quite sure that there was not a research reactor at the Argonne, but there was no production reactor involving heavy water.

Q You spoke of the long range detection matter and the three methods which we speak of rather cryptically. Is it true, Doctor, that it was the opinion of certain qualified people that the one method which you supported might not detect a Russian explosion if it occurred under certain circumstances?

A We argued about that, and I advocated that opinion.

Q That it might not?

A That the Russians might hide an explosion, that this was unlikely, but that they might do it if we relied only on this one method.

Q In other words, the other methods were necessary to make sure that you could detect the explosion?

A That is right. May I add that I know of no instance

in which the method I advocated has not detected the explosion and in which the others have.

Q Do you recall who it was recommended Dr. Libby for appointment to the GAC?

A I wrote a note to Mr. Dean recommending him. Are you asking how the idea came to me?

Q I am asking if you recall who it was, if anyone, who brought his name to your attention?

A Yes, it was Fermi.

Q Did Dr. Pitzer have anything to do with it?

A No.

Q So far as you know.

A I don't know that he had to do with his being appointed, but he didn't discuss it with me.

Q Doctor, you have spoken somewhat of strategic and tactical air power and strategic and tactical uses of weapons and all that; you of course don't conceive yourself to be an expert in war, do you, or military matters?

A Of course not. I pray that there are experts in war.

Q Have you from time to time, however, expressed rather strong views one way or the other in the field of military strategy and tactics?

A I am sure that I have. I don't know what specific views or instances you are referring to, but I am sure the answer to your question is yes.

Q I am not referring to any for the moment.

A I am sure the answer to your question is yes.

Q Doctor, I am a little curious and I wish you would tell us why you felt it was your function as a scientist to express views on military strategy and tactics.

A I felt, perhaps quite wrongly, that having played an active part in promoting a revolution in warfare, I needed to be as responsible as I could with regard to what came of this revolution.

Q To draw a parallel, Doctor, of course you recall that Erickson designed the first iron clad warship.

A I don't. I am reminded of it.

Q Beg pardon?

A I am reminded of it.

Q Do you think that would qualify him to plan naval strategy merely because he built the Monitor?

MR. SILVERMAN: Aren't we really getting into argument?

THE WITNESS: I don't think that I ever planned military --

MR. GRAY: Wait just a minute. Are you objecting?

MR. SILVERMAN: Yes, I think this is argument.

MR. GRAY: Argument?

MR. SILVERMAN: Yes, of course.

MR. GRAY: It seems to me that this Board has



listened for weeks to witnesses who have probed into Dr. Oppenheimer's mind, have said what he would do under circumstances, have stated with certainty what he would , what his opinions are, witnesses who disagreed on this, and I think that counsel has not failed to ask almost any question of any witness that has appeared here. I can't think of questions that could be remotely related to Dr. Oppenheimer that have not been asked.

My ruling is that Mr. Robb will proceed with his question.

THE WITNESS: Now I have forgotten the question.

MR. ROBB: Perhaps we better have it read back.

(Question read by the reporter.)

THE WITNESS: Merely because he built the Monitor would not qualify him to plan naval strategy.

BY MR. ROBB:

Q Doctor, do you think now that perhaps you went beyond the scope of your proper function as a scientist in undertaking to counsel in matters of military strategy and tactics?

A I am quite prepared to believe that I did, but when we are talking about my counseling on military strategy and tactics, I really think I need to know whom I was counseling and in what terms. I am sure that there will be instances in which I did go beyond, but I do not wish to give the

impression that I was making war plans or trying to set up military planning, nor that this practice was a very general one.

MR. GRAY: I think the witness is entitled to know whether Mr. Robb has in mind committees, panels and other bodies on which Dr. Oppenheimer served or something else.

MR. ROBB: I was merely trying to explore in general Dr. Oppenheimer's philosophy in respect of this matter. That is what I had in mind. I was not pinpointing on any particular thing, Doctor, and I wanted to get your views on it as to proper function.

THE WITNESS: I served on a great many mixed bodies. This controversial Vista Project was not a civilian project. There were a great many military consultants. I learned a great deal from them. The formulation of the views of Vista depend to a very large extent on discussions, day to day discussions with working soldiers and staff officers. The committees in the Pentagon on which I sat were usually predominantly committees of military men. I also sat on some bodies where there were no military men. I would have thought that in an undertaking like Vista the joint intelligence, in which I played an extremely small part, of a lot of bright technical and academic people -- not all scientists -- and of a lot of excellent staff officers and military officers was precisely what gave value to the project.

BY MR. ROBB:

Q Doctor, you stated in response to a question by Mr. Silverman that among other things the job of the strategic air power was to destroy enemy aircraft on the fields. Do you recall that?

A Yes.

Q Do you confine the job of strategic air power to that, or would you also include the destruction of enemy cities and centers of manufacture?

A The Strategic Air Command has not only very secret but extremely secret war plans which define its job.

Q I am asking you for your views on its job.

A You mean what it should do?

Q Yes, sir.

A I think that it should be prepared to do a great variety of things, and that we should maintain at all times full freedom to decide whether in the actual crisis we are involved in, this or that should be done. It must obviously be capable of destroying everything on enemy territory.

Q Do you think that it should do that in the event of an attack on this country by Russia?

A I do.

MR. ROBB: That is all. Thank you.

MR. GRAY: I think that the only question I have, Dr. Oppenheimer, really relates to a matter that was discussed

briefly at an earlier appearance before the Board and not anything that has been asked this morning, but I take it that counsel would not object to my question?

MR. SILVERMAN: Anything that will enlighten the Board we are all for.

MR. GRAY: I think I know the answer to this, but there was some discussion about Mr. Volpe, the other day.

THE WITNESS: Yes. I have not read the transcript of that.

MR. GRAY: I don't think this will be involved. Is the Board correct in thinking that this is the same Mr. Volpe that made a speech the other day to the Physical Society?

THE WITNESS: As far as I know, sir. I have not been in communication with Mr. Volpe, but I read it in the newspapers.

MR. GRAY: The Board has discussed this. I think counsel is entitled to know it. The Board has assumed that this was the same man.

THE WITNESS: It obviously is.

MR. SILVERMAN: I should say self evidently the speech was made without our knowledge or consent or instigation.

MR. GRAY: I think I am willing to state for the record that the Chairman believes that this is the case.

MR. MARKS: I wanted to add to what Mr. Silverman said, not only without our knowledge or consent, but to our

embarrassment.

MR. GRAY: I think the Board recognizes that and my question, I would like to have it clearly understood, was not in any suggestion that you as counsel had anything to do with it. My own belief is that you didn't.

MR. SILVERMAN: It is a fact, sir.

MR. GRAY: Dr. Evans?

DR. EVANS: No questions.

MR. GRAY: Mr. Silverman?

MR. SILVERMAN: I have just one or two questions really.

#### REDIRECT EXAMINATION

BY MR. SILVERMAN:

Q Do you think that a scientist can properly do his job of advising the military on the potential of newly developed weapons without having some idea of the use that they are to be put to, and some idea of the tactical and strategic use?

A It depends. I believe we developed the atomic bomb without any idea at all of military problems. The people who developed radar needed to know precisely, or to have a very good idea of what the actual military campaign and needs were. Certainly you do a much better job if you have a feeling for what the military are up against. In peacetime it is not always clear, even to the military, what they will be up against.

Q You were shown two letters by Mr. Robb, one dated September 20, 1944, I think, and the other October 4, 1944. Do those letters in any way modify the testimony you gave on direct examination as to the scale and intensity of the thermonuclear effort at Los Alamos?

A No, no.

MR. SILVERMAN: That is all.

MR. GRAY: May I have that read back?

(Question and answer read by the reporter.)

THE WITNESS: May I amplify? I testified what I could recollect, and I think it is complete, of what was going on at Los Alamos during my period there in the thermonuclear program. I was asked whether these letters caused me to have a different view of what was going on there and I said they did not.

MR. GRAY: I understand, thank you.

Mr. Robb, do you have any questions?

MR. ROBB: I have nothing further.

THE WITNESS: May I make a comment. I don't care whether it is on the record or off.

MR. GRAY: Yes.

THE WITNESS: I am grateful to, and I hope properly appreciative of the patience and consideration that the Board has shown me during this part of the proceedings.

MR. GRAY: Thank you very much, Dr. Oppenheimer.

Do you have anything else?

MR. SILVERMAN: There are two or three documents I would like to have go in. I have no further questions of Dr. Oppenheimer.

MR. GRAY All right.

(Witness excused.)

MR. SILVERMAN: Unfortunately I don't have copies of it here.

MR. ROBB: I don't care.

MR. SILVERMAN: A letter from Major Peer deSilva to Dr. Oppenheimer dated 11 April 1945. I will read it into the record. Do you want to see it first (handing).

MR. ROBB: Sure.

MR. SILVERMAN: "Army Service Forces

"United States Engineer Office

"P. O. Box 1539

"Santa Fe, New Mexico

"11 April 1945

"Dr. J. R. Oppenheimer,

"Project Director

"Dear Oppie:

"Upon my transfer from duty at the project, I want you to know of my sincere appreciation of the support and encouragement which you have personally given me during my services here. In spite of your many more urgent problems and

duties, your consideration and help on matters I have brought to you have been gratifying and have, in fact, contributed much to whatever success my office has had in performing its mission.

"I am sure you know that my interest and thoughts will concern themselves in large measure with the continued progress and ultimate success of the work which you are directing. My service at the project and my association with you and your assistants and fellow workers, are matters which I shall remember with pride.

"I want to wish you and your staff every possible success in your work, upon which so much depends.

"Sincerely," signed "Peer", "Peer de Silva, Major Corps of Engineers.

"cc - Major General L. R. Groves."

During Dr. Oppenheimer's cross examination, Mr. Robb questioned Dr. Oppenheimer about certain public statements that Dr. Oppenheimer had made in which there was reference to the hydrogen bomb.

Dr. Oppenheimer referred to appearing on a radio panel with Mrs. Roosevelt and also to a speech which he made before the Science Talent Search, Westinghouse, I think. We have here the precise thing that was said on those two occasions. I thought I would read them into the record in so far as they relate to the hydrogen bomb, so that you would



know exactly what it is he said.

MR. ROBB: May I inquire as to the source of the text?

MR. SILVERMAN: Yes. I have the text of the radio broadcast in two things. One is the bulletin of the Atomic Scientist, and one appears to be the script of the radio thing. The other, the Science Talent Search thing, is a draft of a talk on the encouragement of science, which comes from Dr. Oppenheimer's files. I understand this was also published in the bulletin of the Atomic Scientist.

DR. OPPENHEIMER: It was published in "Science".

MR. SILVERMAN: I will read what Dr. Oppenheimer said. Other people have said stuff which I don't know is too important. I will read what Dr. Oppenheimer said on the radio thing with Mrs. Roosevelt which appears to have been on February 12, 1950.

"Dr. Oppenheimer: Of course, we personally agree with you about the fostering of science and basic knowledge of nature and man which is one of the few creative elements of our times. It is very essential to the idea of progress to sustain the rest of the world throughout the last centuries. The growth of science is a condition, a pre-condition, to the health of our civilization. It is manifestly not a job for the AEC alone. It is manifestly not a primary job of the AEC or the primary reason for interest in atomic energy. These

reasons lie a lot deeper.

"The decision to seek or not to seek international control of atomic energy, the decision to try to make or not to make the hydrogen bomb, these are complex technical things, but they touch the very basis of our morality. It is a grave danger for us that these decisions are taken on the basis of facts held secret. This is not because those who contributed to the decisions or make them are lacking in wisdom, it is because wisdom itself cannot flourish and even the truth not be established, without the give and take of debate and criticism. The facts, the relevant facts, are of little use to an enemy, yet they are fundamental to an understanding of the issues of policy. If we are guided by fear alone, we will fail in this time of crisis. The answer to fear can't always lie in the dissipation of its cause; sometimes it lies in courage."

That is the end of what Dr. Oppenheimer said on that occasion.

MR. ROBB: Mr. Chairman, might I interpose here for a moment. I have before me what I believe to be what is called in the language of the trade "the off the air" transcript of that statement. I think it is what Mr. Silverman read substantially, but I do find in this "off the air transcript" this sentence at the end of the first paragraph Mr. Silverman read: "It is manifestly not the primary job for the AEC or